of their respective vertices, and some simple trigonometrical formulæ. On p. 173, in this chapter, we notice a curious misprint: in each of three successive formulæ (the usual expressions for the sine, cosine, and tangent of half an angle of a triangle in terms of its sides) a capital V takes the place of the sign of the square root.

The opening paragraph of the first book tells us of the origin of determinants, citing as evidence of their invention by Leibnitz his celebrated letter to L'Hopital, dated April 28, 1693. Their re-discovery by Cramer in 1750, and the rule (for the solution of a system of linear equations) which still bears his name, are next mentioned; but authors of a more modern date are summarily dismissed with the following brief notice:—

"Desde el tiempo de Cramer la teoría de las determinantes ha hecho notables progresos debido á los trabajos de Vandermonde, Laplace, Gauss, Cauchy, Jacobi, Sylvester, Muir, Baltzer y otros, no habiendo rama de las matemáticas en que no haya sido aplicada con ventaja."

We are not, however, left entirely in the dark as to the contributions to the theory made by these writers; for some theorems are called by the names of their respective authors, and a large number of others have these names indicated in brackets. For instance, the proposition which concludes the third chapter in the first book is thus enunciated:—

"Descomponer una determinante de orden $n^{\text{ésimo}}$ en una suma de productos formados cada uno de una determinante de orden $p^{\text{ésimo}}$ y de una determinante de orden $[n-p]^{\text{ésimo}}$ { Laplace }."

This is immediately preceded by—

"Teorema de Cauchy.—Si se elige una fila y una columna de una determinante cualquiera, el elemento común de ellas multiplicado por el respectivo complemento algebraico, más la suma de productos obtenidos multiplicando el producto de un elemento de la fila y de la columna por su respectivo complemento algebraico, es equivalente á la determinante dada."

The way in which these two propositions are treated in the present work will serve to exemplify the methods employed by its compilers for imparting knowledge to their readers. The proof of Laplace's theorem given by Scott, in § 5, chap. iii. of his "Determinants," is clearer than any other we are acquainted with; but it depends on some of the properties of alternate numbers. It is true that these properties are of the simplest kind, but then the notion of alternate numbers is a highly abstract one, quite as much so as the idea of a four-dimensional space. In order, therefore, to convey a clear conception of Laplace's theorem to students of average capacity, our authors have turned it into a problem, and, by considering what Prof. Sylvester calls a simple diagrammatic case, have shown how this problem can be solved, thereby bringing the *theorem* within the grasp of those whose minds are as yet unprepared to revel luxuriously in such abstractions as the alternate numbers.

On the other hand, the proof of Cauchy's theorem and the illustrative example appended to it have been reproduced, with only some slight vertal alterations, from § 62 of Muir's "Determinants," where the theorem in question is presented in a form eminently adapted for elementary instruction.

The first book ends with a rule for the division of determinants, which may be briefly stated thus: To divide $|a_{1n}|$ by $|b_{1n}|$, assume the quotient to be $|x_{1n}|$ and equate each element of the determinant formed by multiplying $|x_{1n}|$ and $|b_{1n}|$ to the corresponding element of $|a_{1n}|$.

The values of the elements $x_{11}, x_{21}, \ldots\ldots x_{nn}$, of the assumed quotient $|x_{1n}|$ will then be determined by solving a system of equations of the form

$$b_{1n}\,x_{1n} + b_{2n}\,x_{2n} + \ldots\ldots + b_{nn}\,x_{nn} = a_{nn}.$$

The article containing this rule should be expunged from all future editions of the work. Its practical inutility becomes apparent when we remember that, on solving the system of equations to which it leads, each $x$ is found in the form of the quotient of two determinants; so that we have to perform many divisions instead of one. Those who are practically engaged in the work of mathematical tuition in the University of Buenos Ayres will doubtless be able to suggest other improvements, and if these suggestions are attended to, students in that University will possess in the second edition of the "Teoría Elemental" an introduction to the theory of determinants written in their own language and suited to their requirements.

In some respects we do not desire to see any improvement. The appearance of the book is as attractive as good paper, wide margins, and a bold clear type can make it. The authors have chosen for their motto the appropriate quotation from Sylvester: "For what is the theory of determinants? It is an algebra upon algebra; a calculus which enables us to combine and foretell the results of algebraical operations, in the same way as algebra enables us to dispense with the performance of the special operations of arithmetic." The table of contents is a model of completeness, and gives the enunciations of the theorems in full instead of merely indicating the pages and articles in which they occur. The volume ends with a selected list of treatises on determinants "que pueden servir de texto y que son dignas de especial mención." This will be of use to students who only want to be told what authors they should read, for the names mentioned are few and well chosen; while those whose object is to improve their acquaintance with the bibliography of determinants may fully satisfy their desire by consulting the two papers by Muir in the *Quarterly Journal of Mathematics* (one of them published in 1881, the other in 1886) to which reference is made.

Responding to the invitation—"agradeceríamos las indicaciones que se nos hicieran sobre omisiones ó errores que no hubiéramos advertido"—we call attention to a slight misprint in this reference, in which the word "Quarterly" has been mis-spelt "Quaterly." With the exception of those previously mentioned, no other *erratum* has come under our notice.

## OUR BOOK SHELF.

*The Geological History of Plants.* By Sir J. W. Dawson, C.M.G., LL.D., F.R.S., &c. 8vo, pp. 290. With Illustrations. "International Scientific Series." (London: Kegan Paul, Trench, and Co., 1888.)

THIS book gives, in a connected form, a summary of the development of the vegetable kingdom in geological time.

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Though likely to be of use to geologists and botanists, the treatment is sufficiently popular to be intelligible to the general reader. The floras of the successive geological formations are treated of in turn, from the oldest rocks down to comparatively recent times. The two longest chapters in the book are devoted to the vegetation of the Devonian and Carboniferous ages respectively, much of the matter here traversed having formed the subject of numerous scientific memoirs by the author. In the body of the work, accounts of the morphology and minute anatomy of the various plant-remains are given, with speculations as to their affinities, and in many cases restorations are attempted, illustrated by figures. The more special details as to classification, &c., are wisely placed in small type as a series of notes at the end of each chapter. The last chapter in the book consists of an interesting essay on the general laws of origin and migrations of plants. Many of the woodcuts leave much to be desired, more especially those dealing with histological subjects. These are, for the most part, scrappy and insufficiently described, and convey little to the mind. Comparisons between fossil remains and recent plants are often rendered valueless by strange inaccuracies as to the morphological value of the parts so compared. Thus the leaves of *Marsilea* (pp. 60 and 67) are described as being in whorls and cuneate in form, and in *Azolla* and *Salvinia* the leaves are "frondose and more or less pinnate in their arrangement." *Spenophyllum*, which possesses wedge-shaped leaves arranged in verticels on the stem, is set down as of probable Rhizocarpian affinity, on this mistaken comparison between its *leaves* and the *leaflets* of *Marsilea!* Much confusion also arises from a careless use of the terms sporocarp, sporangium, macro- and micro-spore, antheridium, &c., in connection with certain small bodies found in the Erian and Carboniferous beds, and conceived by the author to be the reproductive bodies of a rich, then-existing Rhizocarpian flora. Though there are many points in which palæobotanists may not be at one with the author—such as the reference of so many Palæozoic forms to Rhizocarps—the volume will be of service, especially to those to whom the larger treatises are not available.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### Prophetic Germs.

I HAVE but just returned from abroad, and have hastened to read the number of NATURE for August 30. I find that the Duke of Argyll in his letter of that date makes some remarks which call for a few words from me. The Duke is not, it appears, prepared to defend the theory that the electric organ of *Raia radiata* is a "prophetic germ." He refers me to the paper of Prof. Ewart on this subject, whose opinion he quotes and accepts. I am not sure how far Prof. Ewart himself had considered the significance of the view which he put forward in regard to the nature of the rudimentary electric organs of skates; but I do not hesitate to say that there are no facts which have been made known at present, either by earlier observers or by Prof. Ewart, with regard to the electric organ of skates, which necessitate such a theory of prophetic germs as that imagined by the Duke of Argyll, or which can be shown to be inconsistent with the doctrine of progressive development by the natural selection of fortuitous congenital variations. If the Duke of Argyll will point out such facts, he will have made a contribution of some value towards the understanding of the laws of organic evolution.

In a subsequent portion of his letter the Duke of Argyll states: "If Prof. Ray Lankester will explain how 'natural selection' can act upon 'congenital variations,' which he calls 'non-significant'—*i.e.* which are not yet of any actual use—and if he will explain how this action can afford 'the single and sufficient theory of the origin' of (as yet) useless variations, he will have accomplished a great triumph in logic and philosophy."

I am unwilling to entertain the notion that the Duke of Argyll has intentionally constructed the above sentence by garbled quotations from my previous letter in order to produce the false impression that I have maintained such a view as to the action of natural selection. At the same time, I will observe that the method of discussion adopted by the Duke—namely, that of half-quoting the opinion which he attributes to an opponent and desires to render illogical in the judgment of others—is, to say the least of it, objectionable. It becomes easy when this method of partial re-statement is adopted for the disputant to insert words of his own mixed with the words of his opponent, and thus to misrepresent the latter's statement by unconsciously fabricating what the poet has condemned as the worst of fabrications—namely, one which is half a truth.

The point of the sentence which I have above quoted from the Duke of Argyll's letter depends upon the unwarrantable introduction on his part after the quotation of the word "non-significant" of certain words in explanation of that word. The Duke is kind enough to say that by "non-significant" I mean "which are not yet of any actual use." I have not had any private communications with the Duke of Argyll upon this matter, and am at a loss to understand how he should have come to think that he knows that this was what I meant by the word "non-significant." By whatever process he arrived at that conclusion I regret to have to say that it is absolutely erroneous. My meaning was nothing of the kind, and I was under the impression that I had stated with sufficient clearness what my meaning was. It appears that I did not state it clearly enough for all readers. I called the congenital variation which survives in the struggle for existence "non-significant" in regard to its origin and not in regard to its survival. It was, I think, clear to most readers that I was distinguishing between the Lamarckian theory of variation as due to the transmission of parental acquired characters and the Darwinian theory of variation as due to a "shaking up" of the germ-plasma at the union of egg-cell and sperm-cell. The variation—that is, the departure of a young animal or plant from the normal character of the species—would be, if it could be traced to the transmission from a parent of a character acquired by that parent in adaptation to the environment, significant; that is to say, it would have significance for the adjustment of the species in its very origin in the parent. On the other hand, the thousand and one slight or considerable departures from the mean specific form which occur in every possible direction in a brood of young fish or other organisms are "non-significant." They are due to a long-precedent disturbance of the germ-plasma when the form of the organism was undeveloped. No possible reaction of adjustment can be imagined which could produce adaptation in the structure of an animal or plant developed from a germ, if it be a proviso that such adaptation is to have relation to a physical cause of disturbance which once acted upon the germ whilst the adaptational results are to come into effective existence in the developed product of the germ. Hence I am led to speak of congenital variations as "non-significant" in relation to the disturbing causes which produce them.

The proposition that congenital variations are selected when they are not yet of any actual use is an absurdity which the Duke of Argyll had no justification whatever for suggesting as likely to be defended by me, and one which he arrives at by misrepresenting the meaning of the adjective "non-significant." As a matter of course, some one combination of congenital variations is "significant" in the sense which the Duke of Argyll chooses to give to that word—a sense in which I do not employ it: some one combination of congenital variations in each generation survives because it is "significant" in the sense of being useful. It is a common fallacy to suppose that natural selection is only operative in producing *new* species; on the contrary, it is never in abeyance, but is equally as active in maintaining an existing form as in producing a new one.

With regard to the origin of useless variations and the general question of uselessness, it is not to be expected that your columns should be given over to an exposition of the common-places of Darwinism. It is to be noted, firstly, that we have no right to conclude that a structure is useless to the organism in which it occurs because the Duke of Argyll is unable to see in what way

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